Reply to anonymous Referee #3, interactive discussion (C2747-C2755, 2012) on „*The Holocene thermal maximum in the Nordic Seas: the impact of Greenland Ice Sheet melt and other forcings in a coupled atmosphere-sea ice-ocean model*" by M. Blaschek and H. Renssen:

We thank the reviewer for the positive and constructive review, which helps to improve the manuscript and its use to fellow researchers. We would like to reply to the general comments first before replying in detail to the other comments and questions. We agree that the manuscript will benefit from a more detailed discussion on mechanisms and improved versions of figures (included mentioned proxy estimates from the text). Concerning the GIS vs. LIS impact: we would argue that the impact of the GIS is actually quite clear, as the impact remains similar between simulation 9kOGx1, it cools the south-eastern side of Greenland, and simulation 9kOGGIS compared to 9kOGMELTICE. The anomalous warmth in 9kOGMELTICE in the western Nordic Seas can be attributed to an increased convective activity there, as a response to enhanced atmospheric cooling transported east. We agree that the part (3.1.3) where this is explained has to be improved and will hopefully resolve your concerns. A further suggestion is to include into our study another transient simulation with only GIS melt water included, but we would argue that this would not improve the results or the discussion. Please note that our setup can be seen as an update of previous work (Renssen et al., 2009) that involved a stepwise addition of forcings applied to efficiently distinguish impact factors. Additionally this hypothetical simulation would not improve the comparison with proxy data, as the proxies reflect the response to ALL forcings at once (i.e. the separate GIS impact cannot be inferred from the proxy data). Therefore we suggest not to include this additional simulation.

We agree that the LIS has a strong influence on the early Holocene climate and therefore its uncertainty is relevant to this study. We propose to expand the experimental design section (2.2.1) of the paper by a summary of what is included in the supplementary of Renssen et al. (2009), stating changes in the atmospheric circulation imposed by the LIS. These changes are similar to the results of Justino et al. (2006), who investigated the atmospheric response in the LGM climate due to changes in ice-sheet topography with a former version of our model, namely ECBilt-Clio. Their setup includes simulations with the older ICE-4G and newer ICE-5G topography, as well as only albedo effects. Given the quasi-geostrophic approximation used in the ECBilt-atmosphere and its low vertical resolution of only three layers, the cooling patterns of North America and in SSTs over the North Atlantic are similar to results obtained by Kitoh et al. (2001) using the MRI CGCM. Therefore we think that our model, within the limitations set by its idealized setup, gives a reasonably response to topographic changes and produces an atmospheric circulation consistent with theses topographic changes. Following these conclusions we find that our model depends strongly on topography, as it should be, and thus that uncertainties in topographic reconstructions are passed on to our modelled climate (c.f. Shinn and Barron, 1989; Pausata et al., 2011).

1. 5269.22: in what state is the model at quasi-equilibrium? Please document the model drift at this state (e.g. in deep sea temperature, or global ocean T), preferentially with a timeline plot.

   Reply: *It means that the model state is in equilibrium with the forcings. We agree that the use of quasi might be more confusing than helping the reader. The initial thought is that a real equilibrium can never be reached as the model allows variability and as you suggest is recorded by its global ocean temperature. Thus we are not sure if the manuscript will benefit from a plot like this, as it might lead to more confusion than clarification, but we will explain that this was assessed by global ocean temperature.*
2. 5270.21: it is stated that the melt water is added to the surface runoff outlets of GIS. What are these? Are these the modern runoff sites. This is crucial for the model results and should be described in detail, as well as shown in one of the figures. Same applies to LIS melt water.

   Reply: *In our model the runoff points are fixed and correspond to present-day major runoff points from the GIS. The additional melt water is added there. We can describe this detail a bit more carefully in combination with our reply to Referee #1 (#9):*

   We add the additional melt water to the normally calculated surface runoff (e.g. sum of excess precipitation and snow melt), which is then evenly distributed to 10 major runoff points, as for present-day (c.f. Bakker et al. (2012) for locations of the river outflow points).
3. 5272.6: add amount of meltwater used (13 mSv) in 9kOGx1 to the text.

   Reply: *added.*
4. 5273.22: gradients in SST should not be based on different types of proxies. Make clear why this is done, and why it is preferential to comparing gradients in the same proxies (e.g. alkenones, radolaria, foraminifera etc).

   Reply: *We argue that one proxy could be more suitable at one location than another one. Therefore, it could make sense to compare gradients based on different proxies. To clarify this, we expand in combination to our reply to Referee #1 (#15) this paragraph as follows (underlined):*

   There is good agreement between simulated August SST gradients and reconstructed gradients for the diatom-alkenone combination (alkenone SST (east) minus diatom SST (west)). Although this combination might not be as straightforward as using a single-proxy-

gradient, reconstructed values are taken to represent summer SSTs. In our model it is not possible to increase summer SSTs to values as high as eastern SSTs reconstructed by diatoms within the setup of these experiments. A lower gradient compares better to our model results. The gradient is strongest in winter.

5. 5273.23: clarify what is meant by this statement: "Alternatively we could argue for the just diatom gradient, but as eastern SSTs are not as high in the model as the reconstructed ones, we see the other possibility more likely."

   Reply: *Please notice the above reply (#4) which resolves this by rephrasing.*
6. 5274.1: why is 9kOGGIS better than 9kOGx?

   Reply: *Because in 9kOGGIS the impacts of the LIS are included (i.e. it includes ALL forcings) and the gradient is increased as it is recorded by proxy reconstructions, compared to 9kOGMELTICE. Simulation 9kOGx are used as sensitivity experiments, but lack the LIS forcing and therefore comparability with reconstructions.*
7. 5274.5: it is stated in Andersson et al. (2010) that winter SSTs are well represented by the deeper dwelling foraminifera due to the influence of the winter mixed layer. In the model, the largest changes in zonal SST gradients are found in February. Therefore, more emphasis should be put on discussing the model data in light of available planktonic foraminifera data, not solely relying on surface dwelling diatoms (and alkenones).

   Reply: *In Anderson et al. (2010), they also state that deeper-dwelling species are more comparable to annual mean rather than summer or winter, because seasonal variability is negligible below the thermocline. Our results further suggest that impacts from the GIS are likely to affect only the first 50 m of the ocean column, which would limit the impact seen in deeper-dwelling species again. Therefore we think it is valid to focus on surface dwelling species and not to include more foraminifera data.*
8. 5275.20: it is stated that the northward heat transport (PHT) by the North Atlantic is reduced by 68% in 9kOGGIS as a result of reduced meridional overturning circulation (AMOC). Note, however, that there is not necessarily a direct link between AMOC strength and PHT. If this is to be stated here, an analysis of the different components of the PHT must be included. I.e. what is the change in the PHT due to the barotropic (gyre) circulation?

   Reply: *We agree that we have to clarify this point: it is the OHT (ocean heat transport rather than the planetary heat transport).We rephrase: This weakening corresponds to an reduced annual northward heat transport in the ocean at 30 S in the Atlantic basin of 68 % for ...*
9. 5274.25: which convection site(s) is referred to here? Please specify.

Reply: *We rephrase: As a consequence of cooler and fresher surface waters, sea-ice growth is facilitated at the southern tip of Greenland and local convection in winter is reduced (9kOGx1) by up to −200 m of maximum convection depth relative to 9kOG, assisting in an overall cooling effect.*

10. 5277.11: this statement needs clarification: "Alternatively, another yet unknown forcing might have caused the prolonged cooling of the Western Nordic Seas, as in any case it seems to be clear that the impact has to be on the western side, rather than on the eastern side." Why is this impact clear and what are alternative forcing mechanisms?

    Reply: *For 9ka BP the gradient in OGGIS is considerably higher than in OGICE and is outside the 1 STD of simulation OG. The direction of the increase is also opposite in OGGIS compared to OGICE. Therefore we think it is a clear impact. We can see that the speculation was not written in a way that it can be distinguished from previous results and we will improve this part. We speculate that there might have been a period of longer GIS melt and/or an increase of sea-ice cover over the western side.*

    We rephrased in combination with our reply to Referee #1 (#25):

    In order to simulate a more gradual decrease, as in proxy reconstructions (Andersen et al., 2004), GIS melt water could have been longer active. As far as our speculation goes a period of longer GIS melt and/or increase of sea-ice cover over the western side seems possible.

11. 5277.27: this statement needs clarification: "suggesting a west-east spatial timing gradient, rather than a east–west gradient."

    Reply: *We rephrase in combination with our response to Referee #1 (#28):*

    The Eastern Nordic Seas are delayed by 2000 yrs, whereas the delay on the western side ranges between 500 and 2500 yrs, suggesting a west-east rather than an east-west timing difference.

12. Table 2: in the manuscript convection in the Labrador Sea is given much attention as it responds to the input of melt water from GIS and LIS. However, this component of convection is not specified in the table. This should be added, and "North Atlantic", "Nordic Seas" convection should be defined.

    Reply: *We agree that the table can be misleading. We will change the labels and explain more in the caption.*

New References:

Bakker, P.; Van Meerbeeck, C. J. & Renssen, H., Sensitivity of the North Atlantic climate to

Greenland Ice Sheet melting during the Last Interglacial, *Climate of the Past,* 2012, 8, 995-1009

Justino, F.; Timmermann, A.; Merkel, U. & Peltier, W. R., An Initial Intercomparison of Atmospheric and Oceanic Climatology for the ICE-5G and ICE-4G Models of LGM Paleotopography, *J. Climate, Journal of Climate, American Meteorological Society,* **2006**, *19*, 3-14

Kitoh, A.; Murakami, S. & Koide, H., A simulation of the Last Glacial Maximum with a coupled atmosphere-ocean GCM, *Geophysical Research Letters,* **2001**, *28*, 2221-2224

Pausata, F. S. R.; Li, C.; Wettstein, J. J.; Kageyama, M. & Nisancioglu, K. H., The key role of topography in altering North Atlantic atmospheric circulation during the last glacial period, *Climate of the Past,* **2011**, *7*, 1089-1101

Shinn, R. A. & Barron, E. J., Climate Sensitivity to Continental Ice Sheet Size and Configuration, *J. Climate, Journal of Climate, American Meteorological Society,* **1989**, *2*, 1517-1537